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ABSTRACT

Although the nation has been focused on improving its schools for more than a decade, the achievement gap between children from low-income households and those from more affluent households persists. Recognizing the close relationship between education and other supports and services for children, families, and communities, this book is designed to stimulate policy thinking about investments for improving academic achievement. The book contains the following papers: (1) "Can Poor Children Learn More through Improving Their Schools or Making Their Families Stronger?" (John F. Jennings), a foreword introducing the major paper and the four subsequent commentaries; (2) "Finance Fungibility: Investigating Relative Impacts of Investments in Schools and Non-School Educational Institutions To Improve Student Achievement" (Richard Rothstein), arguing that we should pay attention to whether public policies that improve the social and economic conditions of low-income families have the additional effect of improving their children's academic achievement; (3) "School vs. Non-School Investments for School Achievement: Is a Choice Necessary?" (Robert E. Slavin); (4) "Improving the Efficiency of Public Expenditures Directed toward Children" (David W. Grissmer); (5) "The Importance of Non-School Investments for Improving School Readiness and Educational Outcomes" (Ann Segal); and (6) "No-Silver Bullet: Questions and Data on Factors Affecting Educational Achievement" (Thomas F. Pogue). Each paper except the preface contains references. (KB)

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The FINANCE PROJECT

CENTER ON EDUCATION POLICY CEP

Paper by
Richard Rothstein

Foreword by
John F. (Jack) Jennings

Commentaries by
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David W. Grissmer
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IMPROVING EDUCATIONAL ACHIEVEMENT

■

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September 2000

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PREFACE

For more than a decade, the nation has focused on improving its schools. National goals for education have been proclaimed by governors and a president; states and local school districts have raised academic standards for students and have instituted reforms in the training of teachers; and parents have been given more freedom to choose the public schools that their children attend.

Throughout this period, a persistent problem has been the achievement gap that exists between children from low-income families and those from more affluent households. Ever since the schools began to make extensive use of standardized testing, the results have clearly shown that poorer students as a group, although not necessarily as individuals, have done less well in school than have their more advantaged peers.

During the decades of the 1960s and 1970s, serious efforts were made to improve schooling for the poorest children and for youngsters from African-American, Hispanic, and other minority groups that were disproportionately poor. By the early 1980s, those initiatives resulted in a substantial reduction in the achievement gap for many poor students, but today a large discrepancy still exists in the educational achievement among children from families with varying economic resources. Therefore, increased national attention is now being directed to further reducing that gap.

This volume, which is intended to stimulate thinking about investments for improving academic achievement, was produced jointly by The Finance Project and Center on Education Policy. Recognizing the close relationship between education and other supports and services for children, families, and communities, a consortium of national foundations established The Finance Project in 1994 to improve the effectiveness, efficiency, and equity of financing in both of these arenas. The Finance Project's current activities include disseminating knowledge and information to support decision making that produces and sustains good results for children, families, and communities. The Center on Education Policy is the national, independent advocate for public education and for more effective public schools. It communicates with teachers, parents, and members of the general public through presentations, books, pamphlets and brochures, and also encourages actions to improve the schools through assisting states and school districts and through arranging national meetings and conferences.

The volume contains several pieces, each of which reflects the views and interpretations of the author. In his paper, Richard Rothstein asks whether the focus on improving academic achievement through the education system has been too narrow. He asks: Should we so heavily rely on the schools to improve the academic achievement of poor children, or would it be more productive to invest serious sums of money in improving the economic and social conditions of their families? In other words, would we get more bang for our buck in terms of increased student achievement if poor families were to become stronger through various social reforms?

Raising those questions can be very helpful, and we have to thank Richard Rothstein for turning the public debate to addressing that issue. But, clearly, there is not just one perspective on questions of such import, and so this volume includes several commentaries on Rothstein's paper. Those contributors—Robert Slavin, David Grissmer, Ann Segal, and Thomas Pogue—are also due our gratitude for sharing their thoughts on this issue.

The compilation and production of this volume was managed by Carol Cohen of The Finance Project staff. By making the publication available to a wide audience, we hope that it can enrich the public debate on addressing the problems facing poor children and their families, and induce policy makers and leaders to consider the ideas and findings that are presented in the context of their own efforts to improve results for America's children and families.

Cheryl D. Hayes
The Finance Project

John F. (Jack) Jennings
Center on Education Policy

CAN POOR CHILDREN LEARN MORE THROUGH IMPROVING THEIR SCHOOLS OR MAKING THEIR FAMILIES STRONGER?

John F. (Jack) Jennings

The philosophical works of Socrates, Plato, and other ancient Greeks have been a priceless legacy to western civilization, but some modern philosophers assert that a lingering, unfortunate effect of such ancients on today's public discourse are arguments solely for one side or the other of an issue. In other words, we frequently will present a question as a stark alternative, as "either/or." Even the title of this foreword implies such a choice of one or the other option in stating the question as whether poor children can learn more through improving their schools or strengthening their families, when the answer may be that both strategies should be adopted.

In his conclusion, Richard Rothstein says that he does not want his paper to be interpreted as an argument against investing in effective academic interventions in schools. He is not arguing for *either* investing in poor schools *or* changing policies to help poor families. I hope that those who read this volume therefore read his essay with care. This is not an assault on increasing funding for the education of poor children. Rather, Rothstein is arguing that we should pay attention to whether public policies that improve the social and economic conditions of low-income families have the additional effect of improving the academic achievement of the children from those families. The four commentators, although they find some bothersome issues in Rothstein's paper, agree that proper attention has not been paid to that question.

It would be truly unfortunate if a casual reading of Rothstein's paper were to lead policy makers away from correcting the educational deficiencies now facing many poor children. The Education Trust and others have documented that schools with heavy concentrations of poor children are more likely to have inexperienced and uncertified teachers than are the schools of more affluent children. In addition, the majority of states have been sued by poorer school districts alleging unfair systems of school finance that lead to less money often being spent on the education of poor children than on the education of children from higher income families. Bolstering these districts' case, the General Accounting Office has documented that in many states, poor school districts have less to spend than wealthy districts, even though the tax rates of poorer districts are higher than those of more affluent districts. The recent movement by the states to raise academic standards for public schools is leading to an even more intense debate about the educational opportunities available to poor children to enable them to reach these higher standards.

If Rothstein's paper leads to research that shows that the academic achievement of poor children is improved by raising the social and economic status of their families, this is still not an argument that the country should allow substantial inequities to exist in the conditions of

the public schools. No child should be denied a fair opportunity to benefit from a good education solely due to the economic status of his or her family or the community that he or she lives in.

In addition to that question of basic fairness, there is another factor to consider if it can be shown that poor children will do better in school if the condition of their families is improved; namely, does the country have the political will to change public policies to improve the social and economic conditions of poor families? Many of the strategies discussed in Rothstein's paper, such as an increase in the minimum wage, an expansion of the Earned Income Tax Credit, an increase in housing subsidies, and the dispersal of poor, minority families from the central cities into the suburbs, have not proved to be easy policies to enact. If this volume contributes to a change in the public debate so that such policies are researched and found to be effective in raising the academic achievement of poor children, we can only hope that our political and business leaders will have the courage to advocate such desirable policies for the country.

Today, great hopes are being placed on the schools. We expect them to raise the academic achievement of all students, to teach children to become good citizens, and to prepare them for future employment and to become lifelong learners. But children come to school with far different levels of readiness to learn, and the cause of such disparities often is related to the economic and social levels of the children's families. Teachers know about the high expectations of society for the schools and of the differences among children in being able to take advantage of education. Educators should therefore welcome a broadening of the discussion to whether changes in public policy that improve the lot of poor families will also make their children readier to learn. Educational opportunity must be expanded, and we should consider whether improving the conditions of families will greatly assist in that effort.

FINANCE FUNGIBILITY: INVESTIGATING RELATIVE IMPACTS OF INVESTMENTS IN SCHOOLS AND NON-SCHOOL EDUCATIONAL INSTITUTIONS TO IMPROVE STUDENT ACHIEVEMENT

Richard Rothstein

Introduction: A Model of Schools as But One Educational Institution

Schools are but one institution that educates youth. Learning is a product of schools, but also of families, communities, and peers. Social, economic, and cultural forces also affect student achievement. And, of course, academic achievement is also influenced by a student's pre-existing human capital, namely, the tools the individual utilizes to learn, as well as to convert the impact of schools and other educational institutions into acquired skills and knowledge.

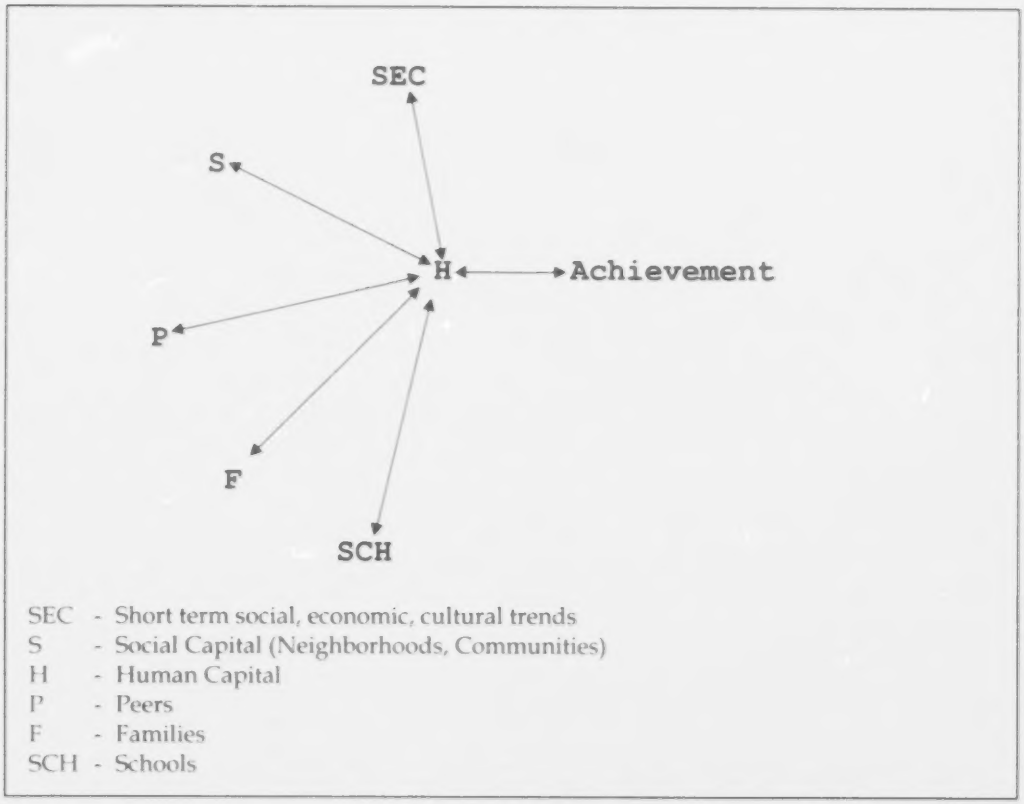
Education researchers and policymakers tend to think of academic achievement as primarily a school effect, albeit one that may be limited or stimulated by other "outside" influences. But an assumption that schools are the primary institutional influence, with others acting only as school modifiers, is without theoretical basis. Rather, each of these institutions can reinforce learning or motivation transmitted by the others, or it can offset the educational influences of the others; on the other hand, each is also an independent educational institution, contributing directly to learning. We do not really know which institution is primarily responsible for academic outcomes; it is likely that different ones may be primary for different students.

Figure 1 illustrates determinants of student achievement. Arrows are bidirectional because students can, themselves, affect how the institutions of which they are a part behave; also, children with different genetic and other human capital characteristics ("H") may react to identical environmental and educational influences in different ways. In Figure 1, schools ("SCH") are an important educational institution, but not the only important one.

James S. Coleman (1988a, 1988b) referred to distinct forms of "social capital." The "social capital of the family is the relations between children and parents," the mechanisms by which parents' (and other immediate family members') human capital is transmitted to children. In Figure 1, "F" ("family capital) refers broadly not only to intra-familial relationships that Coleman described, but to resources that contribute to children's academic growth through these relationships, like the educational level of parents that may affect parental expectations for student performance; the material resources (family income) that support student learning; parenting styles and philosophies (for example, whether parents read to young children, or whether they restrict how much television children can watch); or family cultural values (which may affect, for example, the assumptions that parents make about occupational goals for their children). Family capital, including family resources as well as mechanisms of transmission, may vary within the same family for different children because parenting styles may change as parents age, because parents may react differently to children with different genetically determined traits, or because the intellectual stimulation received by a child may

change as family size increases. Children with fewer siblings may benefit from more family capital than those with more siblings, either because the former benefit from more undivided adult attention, or because older children develop skills in the process of caring for younger ones, and younger children have fewer opportunities for such mentoring (Zajonc 1976).

Figure 1. The Impact of Educational Institutions on Student Achievement



SEC - Short term social, economic, cultural trends
S - Social Capital (Neighborhoods, Communities)
H - Human Capital
P - Peers
F - Families
SCH - Schools

"S" refers to non-family aspects of Coleman's "social capital," again broadened to include the resources of communities in which families live, as well as transmission mechanisms by which communities may influence students' academic performance. For example, social capital can be increased in cohesive neighborhoods where many adults, even those unrelated, contribute to supervision of children. Or, as William Julius Wilson (1987) noted, positive influences in poor communities can weaken if more stable families move to more affluent communities, leaving the remaining children with fewer successful adult role models, or with fewer networks that extend into communities where economic opportunities flourish. The contribution of social capital to educational outcomes may not be linear: neighborhoods with negative (or positive) characteristics may require these characteristics in a minimum quantity before a "tipping point" is reached and they influence student achievement (Crane 1991;

Gladwell 1996).

Peer groups, "P," can also influence student achievement. In African-American communities, peers may pressure children to reduce effort, to avoid "acting white," although a recent analysis suggests that this stereotype is oversimplified (Fordham and Ogbu 1986; Cook and Ludwig 1998). Harris claims that peers have greater influence on a child's personality (including academic motivation) than do parents (1995; Gladwell 1998). Yet the relationship between peers and families is undoubtedly more complex: parents may play an important role in choosing, or in influencing, a child's choice of peers (Evans, Oates, and Schwab 1992).

Broader social, economic, and cultural forces, "SEC," may also impact achievement (although these impacts may be transmitted by temporary changes in the workings of family and social capital, or of peer influences). The apparent decline in academic achievement of American students in the late 1960s and early 1970s was probably caused, in part, by adults' loss of authority during the Vietnam War and the civil rights movement (Wirtz et al. 1977). Schools may not have operated differently, but achievement declined when youths paid teachers less heed. One cause of low achievement by some African-American and Hispanic students may be the expectation that discriminatory labor markets will nullify their academic efforts (Ogbu 1978, 1994). A low unemployment rate can inspire adolescents to stay in school to qualify for better jobs evidently available, or, on the other hand, it could depress achievement if adolescents are seduced by part-time work opportunities (Steinberg 1996), or drop out of school to take full-time jobs. Recent decline in the percent of young males who enroll full-time in college after secondary school may reflect competition of employment opportunities in a strong economy, or it may reflect higher costs of college educations (Lewin 1998). Whichever the explanation, it is likely that these trends are affected more by economic forces than by school effects.

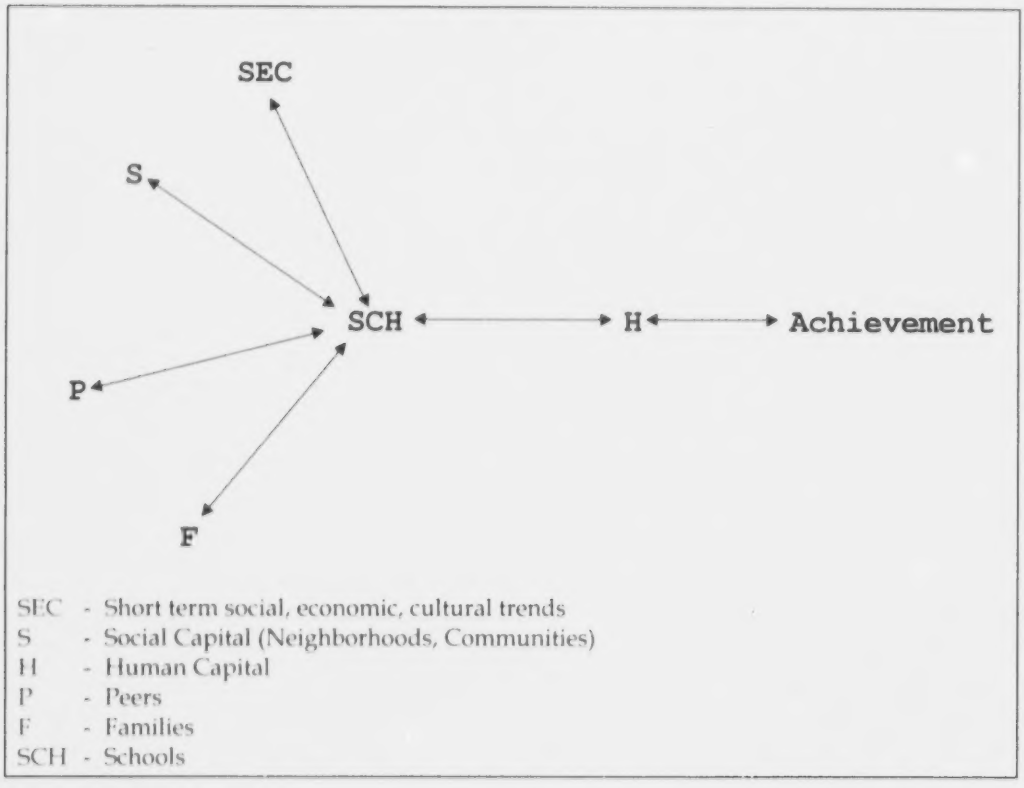
Finally, note that "H," human capital, not only includes an individual's genetic endowment that interacts with each educational institution (families, communities, peer groups, the economy and culture, and schools) in a unique way for each student. Human capital can also be enhanced or diminished by acquired health characteristics: nutrition, exercise, or disease.

In short, families, communities, peer groups, culture, economic markets, and schools are all educational institutions. Changes in any can affect student performance. We know very little about the relative importance of each of these institutions in the contributions they make to achievement. Most analyses today conclude that about 75 percent of the *variation* in student achievement is attributable to student social and economic characteristics. But this should not be confused with a claim that 75 percent of academic learning is acquired outside school. Coleman et al.'s *Equality of Educational Opportunity* (1966) concluded that *variation* in student achievement could best be explained by variation in family and social capital, not by variation

in school practices, but EEO never suggested that schools did not contribute to learning.[1] Most scholars today, even if they believe that the most important influences on relative student achievement come from educational institutions other than schools, concur that schools, if properly focused, can also play an important role.

Most educators and policymakers would probably agree with Figure 1, and with its theory. Yet, we continue to approach education research and policymaking with an implicit theory that differs from Figure 1. Figure 2 illustrates the implicit theory with which most scholars approach these problems.

Figure 2. The Implicit View of Most Educational Research and Policymaking: Controlling for Non-School Influences on Student Achievement



In Figure 2, schools are the sole determinant of student achievement. However, the success

[1] Some researchers have made advances in separating the contributions of these institutions by advanced statistical analyses, but the primitiveness of these analyses is underscored by the fact that different researchers rarely agree on relative importances, primarily because data are inadequate to specify precisely the family, community and other non-school variables that matter (Guthrie and Rothstein 1999). These analyses also are directed at explaining variation between students in achievement, not at explaining the determinants of student achievement itself.

of schools can be affected by families, communities, peers, and broader social forces. Thus, Figure 2 suggests that when we analyze school impacts, we must control for these other factors.

This seemingly small difference in how we conceptualize the institutional roles has dramatic consequences. For example, many policymakers understand that poverty tends to reduce the family capital upon which students can draw and, if concentrated, can also reduce social capital. But because they assume Figure 2-type models of these impacts, they conclude that the remedy is to increase the strength of schools ("SCH") to compensate for the weak positive influence that runs from families ("F") to schools ("SCH"). The conventional way of doing this is to provide additional resources for each child from a poor family to compensate for weak family capital.

But Figure 1 suggests that we should look at the problem differently. If positive influences of families ("F") on human capital ("H") are less than desirable, we could offset this weakness by increasing the strength of the influence of schools ("SCH") on "H." This is the conventional approach. But Figure 1 suggests that if the positive impact of "F" on "H" is weak, then strengthening the influence of "F" itself might contribute to students' human capital and thus to their achievement. Perhaps this is not practical, and the only alternative is to strengthen "SCH" to compensate for weakness in "F." But practicality cannot be assumed; it must be investigated.

Of course, we often adopt policies to strengthen families and communities. But these policies are rarely considered in educational terms. If we want to improve employment prospects of inner-city youth, for example, we consider whether improving their access to good housing might be a way of doing so. But if we want to improve the academic test scores of inner-city youth, we instead only consider which school compensatory programs would be most effective; we rarely consider whether improving access to good housing might, *in itself*, produce higher test scores, perhaps as effectively, if not more so, than some school compensatory programs.

The Relative Cost-Effectiveness of Institutional Interventions

There is ongoing interest in schools' "production function": what resource quantities, applied how, will produce given gains in achievement? Much recent literature focuses on either smaller class sizes, particularly in lower grades and for low-income students (Grissmer et al. 1998); or better qualified (and therefore better paid) teachers (Ehrenberg and Brewer 1995; Ferguson and Brown 1998; Darling-Hammond and Ball 1998). Brewer et al. (1999) calculate that the $5 to $6 billion per year required to fund President Clinton's proposal to hire 100,000 new teachers to reduce grades 1 to 3 classes to 18 would be sufficient, in the alternative, to attempt to purchase higher-quality teachers in those grades by increasing average teacher salaries by $10,000 per year. This analysis challenges policymakers to show that the cost-benefit ratio of achievement gains resulting from one treatment would be greater than those from the other.

Greenwald, Hedges, and Laine (1996), from meta-analysis of school production function studies, calculated the consensus expectation of achievement gains from application of a $500

per-pupil expenditure (PPE) increase to a variety of school improvements. With assumptions such as that present class sizes average 25 and that teacher salaries average $35,000, an additional $500 PPE devoted to lowering pupil-teacher ratios should improve test scores by 0.04 standard deviation units (SDUs); spending it on increasing teacher salaries should increase them by 0.16 SDUs; spending it on increasing teacher experience would increase them by 0.18 SDUs; and spending it on increasing teacher education would increase them by 0.22 SDUs.

A $500 PPE increase represents national spending growth of $24 billion. If the last generation's rate of growth in school spending continues, we can expect to add this amount to public education in about the next 5 years.[2] It is prudent to ask whether new funds should be spent on class size reduction, teacher education, or other school reforms.

But the scope of this question is too narrow. Figure 1 suggests that we want to know not only comparative academic impacts of $24 billion on a variety of school interventions, but of spending those dollars in reform of other institutions. If new school spending of $24 billion can generate, at best, a test score gain of 0.2 SDUs (about 7 percentile points), how does this payoff compare to score gains likely to result from investment of $24 billion each year in the nation's family or social capital, or in economic and social reforms to improve student motivation?

It is puzzling why education research has generally ignored this question. Perhaps disciplinary boundaries of social science discourage full systemic investigations: sociologists query impacts of new resources on social institutions; economists query impacts of new resources on economic production and incomes; and educators, even educational sociologists or economists, investigate impacts of new resources on schools. An interdisciplinary perspective, on the other hand, suggests also investigating impacts of new resources spent in social or economic institutions on *academic* performance.

Instead, researchers too often assume that the task is too forbidding. As Erik Hanushek (1992, 106) put it, "while family inputs to education are indeed extremely important, the differential impacts of schools and teachers receive more attention when viewed from a policy viewpoint. This reflects simply that the characteristics of schools are generally more easily manipulated than what goes on in the family." Considering, however, how daunting it has proven to manipulate school resources to generate dramatically higher achievement, and of the magnitude of resource growth that schools have enjoyed, this assumption deserves review. Twenty-four billion dollars is a lot; it may provide significant levers to affect "what goes on in the family."

Secretary of Education Terrel Bell's 1983 report, *A Nation at Risk*, did much to persuade Americans of school reform's urgency. Assessing his role ten years later, Bell wrote that he "had placed too much confidence in school reforms that affected only six hours [a day] of a child's life... [i]n the face of many negative influences on our children that come from outside

[2] $500 represents about 8.3% of current per pupil spending in public elementary and secondary education in Fall, 1996 (NCES 1997, Tables 2 and 165). From 1966-67 to 1995-96, per-pupil spending, properly adjusted for inflation, increased at an annual rate of 1.7 percent; in the early 1990s, the increase was considerably less (Rothstein 1995).

the school... We have foolishly concluded that any problems with the levels of academic achievement have been caused by faulty schools..." (Bell 1993).

We can avoid this foolishness by reviewing aspects of family and social capital subject to improvement from public investment, describing where such improvements might have payoffs in enhanced academic outcomes, comparable to payoffs from similar investments in classrooms, teachers, or other school inputs.[3]

Family Capital – Income

Higher family income is associated with higher student achievement, but little is known about precisely how income affects achievement. Assuming that the impact of income on achievement can be separated from impacts of other family characteristics that may accompany higher family income (e.g., better educated parents), what do higher-income households purchase that impacts student achievement? Is it better nutrition, health care, or housing, all of which themselves contribute to achievement? Perhaps it is not purchased goods and services themselves that improve achievement, but feelings of self-worth that accompany longer-term higher incomes in our culture. The latter explanation conflicts with American political tradition, which tends to deny the reality of "social class" distinct from the things money can buy. One analysis found, however, that while 5-year average family income had systematic relationship to achievement, there was little association between a single year's income and achievement (Hanushek 1992). This may reflect a social class effect, or may simply reflect that one year's higher income alone may not be sufficient to purchase the goods and services that produce academic gains.

In the early 1970s, the federal government conducted an experiment to determine effects of higher income on a variety of outcomes. Using a negative income tax, randomly selected treatment families received as much as 50 percent more income than control families. The effect on children's test scores was inconclusive, yet in several cases dramatic test score improvements were observed (Mayer 1997); the experiment did not continue long enough to make conclusions about long-term effects of income transfers.

Policy debates about public education focus on the importance not only of improving mean student achievement, but also of narrowing the gap between achievement of socio-economically disadvantaged and advantaged children, and particularly between achievement of African-American and white children. If income does have an academic impact in ways not presently understood, surely educators should be concerned that family incomes of poor

[3] Over 90 percent of school expenditures come from state and local funds, whereas most programs to improve family and social capital are federally funded. This organizational arrangement also contributes to the reluctance of policymakers to consider the fungibility of expenditures to improve academic achievement. But the organizational tradition is not impossible to overcome. Nothing prevents most states from considering family and social investments that could be considered, along with increased per-pupil spending, from revenue growth, and nothing prevents the federal government from expanding the flexibility with which states and localities could spend family, social, or educational grants. Examination of these problems, however, is beyond the scope of this paper.

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(generally lower-scoring) children have declined relative to those of middle class (generally higher-scoring) children. African-American poor children are far more likely to be poor for extended periods, whereas white poor children are more likely to move in and out of poverty; sustained poverty is more highly associated with low achievement than episodic poverty (Duncan et al. 1994). Thus, growth of poverty is likely to put more pressure on the racial achievement gap than on the gap in scores between all poor and all non-poor children.

The ratio of family income at the 20th percentile of a national income distribution to that at the 60th percentile fell from 43 percent in 1973, to 39 percent in 1989, to 38 percent in 1997 (Mishel et al. 1999, Table 1.7). When family income is apportioned on a per capita basis equally to adults and children in each family, apparent growth of inequality in income per child is greater: the ratio of average income per child in families in the bottom quartile of the family income distribution, to that of average income per child in families at the mid-point of the distribution, fell from 60 percent in 1970, to 57 percent in 1980, to 51 percent in 1988 (Fuchs and Rechlis 1992, Table 3). Possibly, if income is related to achievement, then reducing income inequality might be an effective means of reducing achievement gaps between poor and middle-class children.

A long-term decline in wages of workers at the bottom of the wage distribution may have been stopped or even reversed since 1996, as a result of a continued strong economy with its low unemployment rate (CEA 1999, Chart 3.3). If these trends continue and are reflected in greater income per child in poor families, and if this income growth is associated with higher student achievement, educational policymakers should consider devoting greater concern to the importance for education of a strong economy, and to policies that may contribute to it (including not only wage and tax policies, but also macro-economic fiscal and monetary policies). Of course, for most national economic policies, comparison groups do not exist; determining what student achievement might have been, in the absence of certain macro-economic policies, is not possible. However, an inability to calculate the precise impact of, for example, moderate interest rates from Federal Reserve policies on the academic outcomes of disadvantaged children, is no reason to ignore the importance of this impact. If education policymakers are convinced that the impact is real, then their participation in macro-economic debates may be as important for improving achievement as their suggestions about class size, teacher qualifications, or other school reforms.

Duncan et al. (1994) concluded that increasing average (over a 4-year period) family income from the poverty line to twice poverty was associated with a 3.6 point increase in the I.Q. of 5-year-olds. When comparing children from persistently poor families to those from families whose income never (during a 5-year study period) fell below poverty, the I.Q. gap was nine points. In both cases, other family characteristics were controlled, suggesting that money income itself, not other highly correlated characteristics of socioeconomic status (like mother's education, female head of household, etc.) was the important factor. Hill and O'Neill (1994) conclude that an increase in a typical family's income of $10,000 per year in 1988 was

associated with young (about 5½-year-olds) children's test score gains equivalent to 3.9 percentile points. The effect was non-linear, more important at lower income levels.

Mayer (1997) estimates that doubling family incomes from $15,000 to $30,000 per year, in and of itself, would result in an increase in 5-to 7-year-olds' test scores of only 1 or 2 percentile points, with greater increases posted by children whose family incomes are higher for 5 years than children whose family incomes are higher in a single year. She found similar small effects for attainment and teenage childbearing. "Nonetheless," she concludes, "the overall benefit to children from extra income could still be greater than the benefits of any other policy that costs the same" (p. 145). And the effect on young children's test scores could be greater for those whose families are at lower income levels and, of course, the cost of doubling family income for poor families is much lower than the cost of doubling family income for everyone. Mayer also does not distinguish the effects of income gains from different sources. It is possible that effects on test scores differ for different mechanisms: for example, if income is raised by increasing the earned income tax credit for working families, or by increasing welfare benefits.

If a $24-billion national expenditure effected a permanent income increase for the lowest-income families, would resultant achievement gains compare favorably with those expected from a similar expenditure on teacher education or class size reduction? In 1995, 19 percent of all U.S. school-age children lived in families with incomes below the poverty line (about 11 percent of all families had incomes below poverty) (NCES 1997, Tables 20, 21). The average poor family in 1997 had income that was $6,602 below the poverty line; the total national poverty gap (i.e., the amount of money required to bring each poor family's income up to the poverty line) was about $48 billion. The poverty gap for families with children would be somewhat less. Of all poor adults, 67 percent worked in 1996 (Mishel, et al. 1999, Tables 6.10, 6.19). Thus, if an appropriate mechanism could be found, a significant impact on the poverty of families, especially those whose low incomes most negatively impact test scores, could be achieved for a $24-billion annual expenditure, the amount equal to a per pupil expenditure increase of $500 for all children.

Family income is relatively amenable to policy intervention, certainly in comparison to school reform. The real issue is whether we are as confident that income policies will have an impact on student achievement as school policies. One program to supplement incomes of low-income workers is the "Earned Income Tax Credit (EITC)." In 1996, the Internal Revenue Service distributed $26 billion through the EITC program to 19 million low-income taxpayers, three-quarters of whom were families with children (Herman 1998; GAO 1997). Thus, an expenditure of $24 billion, if directed to an expansion of the EITC, would virtually double the size of that program, would make a considerable dent in poverty in America, and might have a beneficial impact on poor children's academic success. We do not now know how much of an impact that might be.

Indirect expenditures to increase family income (and thus family capital) might have a similar effect. There may be a disemployment or inflationary cost to minimum wage increases

beyond modest amounts, but if the disemployment is relatively small compared to the income growth of employed beneficiaries, then we should not dismiss the policy peremptorily. What magnitude of permanent minimum wage increase would be necessary to produce a gain of 7 percentile points in the test scores of children from working poor families? If the cost in higher inflation or lower economic growth is comparable to the $24 billion that we may be prepared to spend on class size reduction or teacher education, then the alternative of a minimum wage increase may be reasonable.

At first glance, this cost seems prohibitive. But if a $10,000 annual family income increase (or a wage increase of $4.81 per hour for a full-time year-round worker) is necessary at median family income to produce test score gains of 3.9 percentile points, it is possible, indeed likely, that lesser income increases would be needed for similar academic gains, or greater academic gains would result from similar income increases, at lower income levels. Because minimum wages are often raised at the state level, as well as federally, natural experiments can be observed to determine the effects of an increase. David Card and Alan Krueger, in *Myth and Measurement* (1995), shattered the economic consensus that modest minimum wage increases necessarily entailed job losses. By comparing states that enacted minimum wage increases with neighboring states that did not, Card and Krueger cast strong doubt on the inevitability of disemployment from minimum wage increases; they suggested that, on the contrary, modest increases could create incentives to improve productivity in ways that increased employment.

Could similar natural experiments be observed to determine if test score gains result from minimum wage hikes? This would be more difficult. Minimum wage differentials between neighboring states might be insufficient to affect family capital that contributes to learning. Differentials would likely have to persist for several years to affect family capital, unlike the case of disemployment effects, which might be expected to show up quickly. And Card and Krueger could usefully compare neighboring states in investigations of employment effects, because they could identify neighboring states where most economic policies and conditions were similar except for minimum wages. In education, however, it would be extraordinarily difficult to control for the other institutional influences on achievement of children from working poor families, while observing impacts of income increases alone. Nonetheless, exploratory investigation is certainly warranted into the question of whether income increases for such families would have a comparable impact on their children's academic achievement to that of additional investments in schools. Unmentioned in this discussion, of course, are possible collateral social benefits of income-support policies, not test scores alone.

Family Capital – Nutrition

What if, instead of spending funds for undifferentiated income support to the poor, policy instead targeted expenditures to subsidize particular goods or services that poor families most need to enhance their children's achievement? Because we do not presently understand the mechanism(s) for translating family income into academic achievement, it is difficult to know

how to spend such funds, but we have some clues.

In experimental studies, children given vitamin and mineral supplements "showed test score gains that significantly exceeded the controls" (Neisser et al. 1996, 88-89). Because nutritional supplements are relatively inexpensive, this is a particularly productive area for investigation of relative cost and benefit. Further experiments are warranted.

One study found significant association between test scores of inner-city kindergarten children, and the deviation of these children's weights from normal weights for that age and gender (Karp et al. 1992). Most provocative in this study was that, at least for African-American inner-city children, weight was more strongly correlated with tests of school achievement (the Stanford Achievement Test) than it was with measures of cognitive or visual-motor-perceptual ability. This finding suggests, but does not prove, that nutritional deficiencies in young children might not cause as much permanent as temporary damage, and thus that direct nutritional subsidies to poor families and their children might be alternative ways to spend funds effectively to improve achievement.

Family Capital—Prenatal Care

There is an association between very low infant birthweights and poor academic performance. We should calculate the national investment in prenatal care required to produce achievement gains equal to given investments in classrooms or teachers. Perhaps relatively small investments in the nutrition of low-income pregnant women would generate test score gains.[4] In one experiment involving mothers believed to be at-risk for delivering low birthweight babies, babies born to mothers given liquid protein dietary supplements exhibited more behaviors associated with later test score gains than did babies born to mothers in a control group. But there has been no definitive long-term follow-up of this experiment (Neisser et al. 1996, 89).[5]

One intervention almost certain to have impact is reduction of smoking by pregnant women. Smoking by pregnant women has been found to cause an approximate mean decrease of 4 points in their children's IQs (Drews et al. 1996). This is a big decrease and would have substantial impact on later test scores. While the harm to fetuses caused by smoking of their mothers when pregnant receives wide attention in America today, with notices on every cigarette package, the payoff for further reductions is great; they should be a continued policy focus to improve academic achievement.

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[4] We currently spend for this purpose through the "WIC" (Women's, Infants' and Children's) nutrition program. The question here is whether additional expenditures, in perhaps a different form, might increase the effectiveness.

[5] As this report went to press, a study just published in the *American Sociological Review* suggested that the academic effects of low birthweight were much greater than previously understood. Dalton Conley and Neil G. Bennett found that babies with below-average weights at birth were 34 percent less likely to graduate on time from high school and 74 percent less likely to graduate on time than their normal-birthweight siblings. This report has relied upon a newspaper account (Viadero) for a description of the Conley-Bennett study; the study was not reviewed directly.

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Family Capital – Lead Exposure

The Centers for Disease Control recently concluded that 4.4 percent of the nation's children aged 1 to 5 continue to have harmful blood lead levels (GAO 1999). Eight percent of children in Medicaid-enrolled families and 12 percent of those in families receiving WIC supplementary food benefits had dangerously high levels, sufficient to "cause reductions in IQ and attention span, reading and learning disabilities, hyperactivity and behavioral problems."[6] According to a 1993 report of the National Research Council of the National Academy of Sciences, further study may result in lowering the threshold for presuming dangerous exposure; thus, considerably more than 4.4 percent of the nation's children may have learning capacity reduced from lead poisoning. Yet fewer than 20 percent of children most at-risk of lead poisoning, those in low-income families, have been screened for dangerous levels of lead (GAO 1999, 14, 28). Screening rates vary widely between states, in part because, while federal guidelines permit Medicaid coverage for lead poisoning screening, not all states take advantage of this regulation. State policy variation invites observation of natural experiments here.

Children most at risk of having dangerously high lead blood levels come from the same family income groups whose children characteristically score below national norms on academic tests and whose other achievement outcomes are low. These are also the children for whom class size reductions are most indicated. It would seem reasonable for education researchers to estimate whether funds spent on lead poisoning screening, and on subsequent remediation and treatment (primarily by identifying and removing sources of lead), might produce achievement gains comparable to those from class size reduction or other school interventions for low-income children. Such comparison might be particularly fruitful because maintenance of reduced class sizes incurs ongoing expenditures. But if existing environmental lead sources can be removed, then the costs of future screening and removal could be reduced without loss of academic gains realized from such a program.

Family Capital – Health Care

Poor children are more likely to be admitted to a hospital and have longer stays, once admitted. Twice as many had iron-deficiency anemia in the early 1980s than did non-poor children. Poor children were two to three times as likely to have severe vision impairment (Starfield 1982). These indicators may subsequently have improved slightly, but in 1991 poor children were still losing 30 percent more school days than non-poor children (Starfield 1997). These conditions affect academic achievement.

Greater access to health care could ameliorate such problems, and contribute toward equalizing academic achievement. Fifteen percent of all children, nearly 11 million, are without

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[6] The consensus that lead exposure has "well-established negative effects on intelligence" (Neisser et al. 1996, 89) is not unanimous. The magnitude of effects of exposure to lead "fluctuates oddly from one study to the next and is still the subject of dispute" (Neisser 1997, 79). This, of course, makes the impact of lead exposure on student achievement no less certain than the impact of any school intervention. Further research is needed.

health insurance; the percentage of poor children without health insurance is probably much higher – 32 percent of the poor (of all ages) lack health insurance (Pear 1998). If greater access to health care would produce greater health in poor children, then academic outcomes could improve. Uninsured children have 25 percent more school absences, and are more likely to have undiagnosed and easily remediable problems (like vision and hearing impairments) that affect academic achievement.

In 1989, the federal government designed a Florida pilot project in which school districts purchased group health plans for their uninsured students. Enrollment opened in 1992 and in 1995 the federal demonstration project ended; Florida then continued the program with local and state funds. Family premium contributions were required, covering 35 percent of program costs. In 1996-97, monthly premia for children from families with incomes below 130 percent of the poverty line (i.e., the children were free-lunch eligible) ranged from $5 to $10, depending on contracts negotiated by school districts with commercial health plans. These were 70 percent of all covered children. Premia for children from families whose income was between 130 percent of the poverty line and 185 percent of poverty (i.e., the children were reduced-price-lunch eligible) ranged from $13 to $25. Premia for children not enrolled in the lunch program ranged from $48 to $53. The program cost the state and local districts $16.7 million in 1996-97, for 37,000 children, 3.4 percent of Florida's school population in the 16 counties that participated in the program that year (FHKC 1997). Averaged over the entire school population in these counties, the subsidized cost was about $15.18 per pupil.

After 1997, the program was expanded with new funds from Title XXI of the Social Security Act (the Children's Health Insurance Program, or "CHIP") and from Florida's settlement with tobacco companies. By 1999, 63,000 children in 41 counties were covered; eligibility for subsidies was increased to children from families with incomes up to twice poverty (FHKC 1999). Even if all uninsured children in Florida were enrolled (about 245,000 children), the overall cost would be unlikely to exceed $100 per pupil. The state estimates that its 1996-97 subsidy was fully offset by reduced emergency room visits by children who now had primary care physicians and preventive care. (Some of this offset was realized by private charity hospitals, not the state itself.) But there has been no measurement of possibly improved academic outcomes.

The most common illnesses diagnosed by participating physicians were asthma and attention deficit hyperactivity disorder (ADHD). Each, untreated, causes serious academic problems, the former because of significant absenteeism. While there are good reasons to be concerned with children's access to health care beyond school effects, it seems reasonable to ask if average achievement gains that result from such a program compare to those that could be generated by other objects of per pupil spending increases.

Seven other states had begun planning similar programs before the adoption of Title XXI in 1997, and now all states receive federal funds to do so. Because the programs must be implemented gradually, careful experimental-design research projects could compare the

academic effects for children who are enrolled to those for children not (or not yet) enrolled.

Another approach, more common prior to adoption of the federal CHIP program, is to staff schools in disadvantaged communities with nurses and clinics. The National Association of School Nurses recommends a nurse-to-student ratio of 1:750, and 1:125 for severely chronically ill or developmentally disabled populations (Robinson 1999). Utilizing an overall ratio of 1:750, the per-pupil cost of staffing schools with a nurse would be about $80 per pupil. If nurses were concentrated in schools with disadvantaged populations, then it seems reasonable that achievement gains would be one outcome. It should be possible to research the comparative impact on student achievement of schools that have nurses and otherwise similar schools that do not.

A new report by the United States Surgeon General finds that more than a third of all poor children aged 2 to 9 have untreated dental cavities, about twice the rate for non-poor children. In response to such crises, the dean of Columbia University's school of dentistry has established dental clinics at middle schools in low-income neighborhoods in Manhattan. "These are kids that are sitting in class with toothaches. This is a problem, and it's neglected," said the dean, Dr. Allan Formicola. Education researchers, however, have little understanding about precisely how much of a test score gain can be expected from such a simple and low-cost intervention.[7]

James Comer and colleagues have suggested that family capital (such as attitudes and cultural assumptions) of low-income African-American children influences their academic achievement. Comer's "school development program" (SDP) focuses on children's physical and mental health to facilitate their adjustment and that of their parents to the culture and assumptions of learning in a school. Assessments of the impact on academic achievement of SDP investments in mental health professional services is still inconclusive (Herman and Stringfield 1997).

Family Capital—Parental Age

Children born to teen mothers are usually at risk of academic failure. Grissmer et al. (1994) concluded from NELS and NYLS data that the mother's age at a child's birth was an important predictor of student achievement. This aspect of family capital's impact on student outcomes presumably arises because immature mothers make lesser contributions to their children's achievement than do mature mothers. Yet other analysts have concluded that, for poor African-American teen mothers in particular, lower age at a child's birth may be associated with higher achievement (Geronimus 1997). This counterintuitive finding may reflect the fact that, in very poor black communities, children born to teens will more likely be raised by grandmothers or other extended family members, while those born to equally poor mothers in their 20s may be

[7] The Surgeon General's report (*Oral Health in America: A Report of the Surgeon General/Third National Health and Nutrition Examination Survey*) was issued as this report went to press, and it was not reviewed directly. This report has relied upon a newspaper account (Stolberg) for a description of the Surgeon General's report and for the dental clinic program organized by Dr. Formicola.

raised without as much benefit of extended family support. It may also reflect that, for the very poor, health deteriorates as women age from the teen years to the twenties, so children born to women in the latter group have less healthy mothers. A high priority for research, therefore, is to reconcile these possibly conflicting views about the impact of teen motherhood on achievement. In the case of children for whom teen motherhood has adverse consequences, we should ask what investment in adolescent sex education and mental health would be required to produce academic gains equal to those produced by investments in class size reduction or teacher training. Only a small portion of the $24 billion that we speculate may be destined for class size reduction or teacher qualifications, if invested instead in adolescent health clinics, could go a long way towards producing student academic gains, by raising average ages for mothers.

Family and Social Capital—Housing

As noted, abnormally low weights-for-age, a result of nutritional deficiencies, seem to be associated with poor academic performance. One study found that children from families who received housing subsidies (for example, through the federal Section 8 housing program[8]) were less likely to have abnormally low weights for their ages than were children from families who were on waiting lists for such subsidies. Families with housing subsidies spend more of their incomes on food than eligible families without them. Subsidies apparently permit families to redirect income to nutrition, avoiding low-weight crises and consequent depressing effects on academic achievement (Meyers et al. 1995).

Housing subsidies may also improve outcomes by making affordable lower-cost apartments with more adequate space for children's study or homework. Subsidies may also impact achievement by stabilizing living arrangements. If, as seems probable, eligible families tend to be those who move frequently, then housing subsidies may permit their children to remain in the same schools. Student mobility may be an important cause of low achievement in poor communities: "Moving generally keeps children of lower SES from attaining their normally expected achievement and grade level" (Wang, Haertel, and Walberg 1994). About one-sixth of all third-graders nationwide have attended three or more schools since first grade. These frequent movers are disproportionately low income and minority: 30 percent of children from families with incomes below $10,000 have attended at least three different schools by the third grade, while only 10 percent of children from families with incomes above $25,000 have done so (GAO 1994, 5). An analysis of Chicago mobility found that in a typical elementary school, half the students are not enrolled in the same school after 3 years; over a 2-year period, 5 percent of Chicago students attended four or more different elementary schools; three-quarters of these "frequent movers" were African-American (Kerbow 1996). In the Los Angeles Unified School District, the average annual elementary school transiency rate (students who

[8] In the "Section 8" program, families can receive subsidies equal to the difference between 30 percent of their incomes and "fair market value" of their rental units. Participating landlords must charge only the "fair market value."

enter or leave school during a single year) was 43 percent in 1989-90, and at one site it was 96 percent (Bruno and Isken 1996).

Mobile students lose continuity of instruction, lose familiar peer relationships that provide security for learning, and cannot take easy advantage of remedial programs for which diagnoses emerge over time, or programs for which eligibility must be established. For example, mobile students are less likely to receive Title I reading services for which they would be eligible (GAO 1994, 12). Not only are test scores depressed and dropout rates elevated for mobile students themselves, but achievement of all students in high-mobility schools suffers because teachers must devote instructional time to reviews for newcomers and to organizational tasks of incorporating them into classrooms. Schools with high mobility rates frequently reconstitute classrooms to avoid placing all newcomers in a single class. In one typical inner-city school, a research team observed class reconstitutions, affecting stable as well as mobile students, four times in a single academic year (Bruno and Isken 1996, 243). Even without such reorganization, teaching strategies deteriorate in schools with high mobility: teachers are more likely to teach discrete units, rather than integrating instruction across subjects, and are more likely to spend time reviewing old material than introducing new units (Kerbow 1996). The movement in American education for greater accountability is also frustrated in schools with high mobility: stable students may make achievement gains that are disguised by including in school averages the test scores of recent arrivals for whose achievement the new school is not fully responsible.

Student mobility can have several causes, including dislocations from parental job loss or transfers, from school choice programs, or from family breakup or reorganization. But in low-income communities, an important cause is almost certainly inadequate housing. A serious housing shortage exists for low-income urban families, and many families with children intermittently double-up with friends or relatives, or move when they cannot keep up with rent payments. In extreme cases, families move in and out of shelters or other non-standard housing.

Of 1.7 million renter families with children in 1995 who were "working poor" (i.e., income from work equalled at least half-time year-round work at the minimum wage), 80 percent spent more than 30 percent of their incomes for rent and utilities, and 42 percent spent more than 50 percent of their incomes for rent and utilities. One-third of these families with children either lived in overcrowded housing or were doubled up with other families (Daskal 1998, 28).

If families acquire stable housing in less distressed neighborhoods, a child's social capital may improve along with family capital. Or social capital may improve, feeding back in turn to further improvement in family capital. If stable housing is located in safer neighborhoods, then parents may feel more secure about leaving children at home when the parents leave for work. If this security improves parents' work reliability and employability, then family income could increase, resulting in further capital improvements that contribute to learning.

A long history of research on effects of student mobility on achievement almost invariably leads policymakers to consider how schools can accommodate to special needs of mobile students: by improving the speed with which student records follow school transfers, providing busing so that students can avoid changing schools when families move, or by offering remedial tutoring for mobile students (GAO 1994; Kerbow 1996). Some educators consider whether mobility itself can be reduced by urging parents to consider the impact on their children's achievement before deciding to move, and some school policymakers have attempted such an approach (Schuler 1990). Educational research, however, rarely considers whether directing resources to reduce mobility directly, by means, for example, of housing subsidies, may be a cost-effective approach to improving test scores, quite aside from other beneficial social results of such subsidies.

Publicly funded housing subsidies are necessary to reduce student mobility. New York City families housed in homeless shelters who received housing subsidies within the next 5 years were 21 times more likely to be stable (in the same apartment for at least 12 months) than families who did not receive subsidies (Shinn et al. 1998). Nationwide, only one-quarter of working poor families with children received public housing assistance in 1995; 569,000 working poor families received no housing assistance and paid more than 50 percent of their incomes for rent and utilities; another 463,000 paid between 30 and 50 percent of their incomes (Daskal 1998, Table 1). The Department of Housing and Urban Development estimated in 1997 that the average cost of housing subsidies was $5,499 per year per unit (GAO 1998, Table 3.1). Thus, an immediate expansion of the Section 8 program, to cover all working families with children who presently spend more than 50 percent of their incomes for rent, would cost only $3.1 billion annually. Expansion to cover all of working families with children presently spending more than 30 percent of their incomes for rent would cost $5.7 billion annually. These amounts should be considered in the context of the heretofore described expected increase in elementary and secondary education spending of $24 billion, much of which will undoubtedly be directed at the most disadvantaged children, the very ones whose families lack adequate housing. An annual housing expenditure of $5.7 billion is equivalent to a per-pupil spending increase of about $121, spread across all elementary and secondary pupils. No research has yet been undertaken to determine relative gains in student achievement from the expenditure of $5.7 billion in Section 8 housing vouchers, as opposed to class size reduction or recruitment of more qualified teachers, but the possible results are sufficiently provocative that such research should be a high priority.

Housing Experiments

Experimentation could elucidate the impact of housing policy on academic achievement. After years of ignoring calls for such studies (by, for example, Gilbert and Mosteller 1972), education researchers have developed renewed interest in controlled experimentation as a superior means of detecting impacts of specific resource treatments on achievement. Because

specification of family, community, and economic variables is so imprecise, nonexperimental studies that parse responsibility for achievement between schools, families, and communities are likely to continue to be inconclusive, at best, or contradictory, at worst. This is also true of so-called "natural experiments" (like the Card-Krueger analysis of state minimum wage increases) because it is so difficult to identify control groups that are "identical" in all relevant respects to treatment groups. Two true educational experiments, involving random assignment of children to treatment and control groups, have therefore gained wide recognition: the Tennessee class size and Perry pre-school studies. In the former, students were randomly assigned regular classes, reduced-size classes, or regular classes with paraprofessional aides. Researchers found that reduction in class size from about 22 to about 15 produced score gains, with greater gains for minority than for white students (Mosteller 1995). In the Perry study, low SES African-American 3-year-olds were randomly assigned in the mid-1960s to receive services such as teacher home visits. Followup studies during the next 25 years found higher academic scores, better graduation rates, and higher marriage rates, along with lower welfare utilization and criminal activity for the experimental group that had received the pre-school services (Karoly et al. 1998). Replicating these experiments, as well as designing others, should now be a high priority. "If we did more experiments, we might eventually develop better theories" (Jencks and Phillips 1998, 42).

Urban policy researchers have also renewed their interest in experimentation, and for similar reasons. Impetus also came from descriptions of Chicago's "Gautreaux" housing program that seemed to show that African-American public housing residents who utilized federal housing vouchers to move to suburban rental units had better employment outcomes than comparable families who utilized vouchers for rental units in the city. Adolescent children of the suburban movers also apparently fared better than their urban counterparts, having lower dropout rates and better academic achievement (Rosenbaum 1991; Kaufman and Rosenbaum 1992). James Rosenbaum, a Northwestern University professor mostly responsible for publicizing the Gautreaux program, concluded that although grade point averages of suburban and city movers were similar, these grades probably represented higher achievement in the suburbs than in the city because suburban schools had higher standardized test scores.

If sustained, Rosenbaum's conclusions (the importance of providing integrated suburban opportunities to inner-city African-Americans) can be important for educational policy. As noted, poverty's effect on African-American children is likely to be greater than on white children, because African-American children more likely come from families where poverty is sustained, not episodic. And poor African-Americans are increasingly concentrated in inner-city areas (Gramlich et al. 1992; Kasarda 1993). Here, social capital is lacking, even after low income (family capital) is controlled. Thus, even if black poverty did not increase relative to white poverty, to the extent that social capital contributes to student achievement, the achievement gap between white and African-American children could grow. Improvement in the quality of housing for poor African-American families might not be sufficient, for

educational purposes; ways must be found to integrate these families in more middle-income communities as well. Rosenbaum's research promised to provide new data to support the importance of integration as a housing strategy, not merely the improvement of housing conditions within African-American ghettos, if racial gaps in children's achievement are to narrow.

Rosenbaum asserts that the Gautreaux program was nearly a true randomized experiment, because apartments in both suburbs and city were offered to eligible families in an unsystematic fashion. While eligible families (each was first screened for responsibility and good renter behavior patterns) could, in theory, refuse a voucher for either a suburban or city apartment, in practice they were under pressure to accept the first rental offer presented to them, whether suburban or urban, because there was a long waiting list for subsidized units and families could not expect to get a second offer in the near future, if at all (Rosenbaum et al. 1993).

It is doubtful, however, that this practical reality converted the selection process for Gautreaux suburban movers into a true random assignment, in which there were no systematic differences between those who accepted suburban offers and those who did not. And Rosenbaum's Gautreaux results also could not control for attrition—some families who accepted suburban vouchers chose not to remain, and moved back to the city. The Gautreaux results whetted the appetites of housing experts for a true experiment, and in 1994 Congress appropriated funds for the Department of Housing and Urban Development to implement "Moving to Opportunity" (MTO), an experiment to determine whether low-income families benefit from living in communities with greater social capital.[9] Experimentation is rarely possible in social science, because it necessarily requires granting a benefit widely believed to be valuable (i.e., better housing) to some participants and not to others. It also requires a degree of social engineering with which policymakers justifiably feel discomfort.[10] The denial of a benefit to controls usually presents the most difficult ethical problems, but these are mitigated if the benefit is scarce due to no fault of the experimenters, and the experimental pool from which both treatment and control groups are drawn can be comprised entirely of volunteers. The benefit can then be randomly allocated, lending itself to observation of an experiment.

These conditions are met in the MTO experiment, because in all major cities, waiting lists for subsidized private apartments far exceed the supply, so establishing a control group whose members do not receive subsidies does not withhold benefits from those who otherwise might

[9] MTO differs from Gautreaux; MTO tests effects of moving from low-income communities, whereas Gautreaux tests effects of moving from minority communities. In practice, there is considerable overlap.

[10] "From a scientific viewpoint, the best way to estimate neighborhood effects would be to conduct controlled experiments in which we assigned families randomly to different neighborhoods, persuaded each family to remain in its assigned neighborhood for a protracted period, and then measured each neighborhood's effects on the children involved. Fortunately, social scientists cannot conduct experiments of this kind" (Jencks and Mayer, 1990).

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receive them but for their participation.[11] The Section 8 housing subsidy program is not an entitlement, but is limited by congressional appropriation: a family eligible for Section 8 housing vouchers waited an average of 2 years to receive subsidies in 1995-96. In many cities, waiting lists are so long that they have been closed; new families are not permitted to apply (Daskal 1998, 5). The wait for a voucher is now 10 years in Newark and Los Angeles, 7 years in Houston, and 5 years in Chicago and Memphis. The problem is compounded because in tight housing markets, landlords are unwilling to accept vouchers, and so some families that finally receive vouchers are unable to find apartments where they can be used (Janofsky 1999).

The MTO experiment establishes lists in five cities (Baltimore, Boston, Chicago, Los Angeles, and New York) of families with children who presently live in public housing or in subsidized privately owned low-income projects located in high-poverty neighborhoods (i.e., census tracts whose poverty rate exceeded 40 percent in 1989). To get on the lists, families must express interest in utilizing vouchers to move to private (non-project) apartments in low-poverty (middle class) communities, defined as census tracts where poverty was less than 10 percent. MTO officials randomly select from these waiting lists for three groups: the main treatment group receives vouchers for subsidies to rent private apartments in low-poverty communities, and these families are given extensive counseling and assistance in locating such apartments; a comparison group receives vouchers for subsidies to rent private (non-project) apartments in any census tract where they can find such apartments without counseling and assistance; and a third group, the control, receives no vouchers for private (non-project) housing. Scholars were invited to track the experiment over a 10-year period, and report on the results in each of the five cities.

Because urban policy scholars are familiar with debates about family and social capital stemming from William Julius Wilson's (1987, 1991) "truly disadvantaged" thesis, and because MTO was designed when national concern with "underclass" crime and unemployment was more pressing than low academic achievement of inner-city youth, most analyses springing from MTO concern whether, once living in middle-class communities, MTO's treatment families were more likely to be employed, less likely to be victimized by crime, and less likely to apply for and receive other public assistance. However, a few MTO studies address educational issues.

Ludwig and Ladd (1998), for example, are comparing the academic "value added" in suburban Baltimore schools attended by experimental group children to that provided in schools attended by comparison and control group children, and find that the suburban schools

[11] In other respects, however, the program has been controversial. Particularly in Baltimore, groups claiming to represent suburban residents complained that moving poor families into the suburbs would raise crime rates and reduce property values. As a result, HUD delayed the experiment, and then scaled it back (Dreier and Moberg 1995; Gordon 1997). Heather MacDonald (1997) of the Manhattan Institute attacked the program, claiming that recipients of vouchers who move to the suburbs will include not only the victims of inner-city social disorganization, "but the perpetrators as well,who may then spread social problems to marginal but stable working-class neighborhoods."

do not provide greater value added, after controlling for student composition. However, the conclusion is tentative and differs from one reached in a previous paper (Ladd and Ludwig 1997). Katz, Kling, and Liebman (1997) find that preliminary data in Boston suggest that experimental and comparison group children are more likely to read for enjoyment, but less likely to participate in extracurricular activities, than control group children. Robert Crain and Joe Darden are studying educational outcomes of MTO families in New York (Goering 1996), while Sara McLanahan and Maria Hanratty are exploring the development of test score data for children of MTO experimental, comparison, and control group families in Los Angeles (Hanratty 1999).

There is much unexplored ground here. Opportunities for observation of educational experiments in MTO far exceed those yet identified. Do test scores of children in MTO treatment groups exceed those of the comparison or control groups? If so, is this because these children now have smaller classes and/or better qualified teachers, or because they now encounter higher expectations from peers and neighbors? Or, on the contrary, does placing treatment children in suburban communities lead them to withdraw from academic competition from feelings of inadequacy, as Jencks and Mayer (1990) have noted may sometimes be the case?

Moving to Opportunity is slightly more expensive than regular Section 8 programs, because finding suburban housing for inner-city families and successfully managing their moves require counseling and training of the families, and negotiation with landlords. In Gautreaux, additional counseling and landlord negotiation costs, above typical Section 8 spending, are "less than" $1,000 per family; for other similar programs, the range is $950 to $1,650 (Davis 1993; Goering et al. 1995). Yet given the possibly low cost of housing subsidies for the poorest working families with children, relative to what we will spend in compensatory school programs for the same children, study of MTO experimentation has great importance.

There are smaller programs like Gautreaux now in operation in Boston, Chicago, Cincinnati, Dallas, Hartford, Las Vegas, Memphis, Milwaukee, and Yonkers (Goering et al. 1995; Dreier and Moberg 1995). Some of these may also provide data for study of the impact of housing change on academic achievement. But even if MTO or Gautreaux-type studies show dramatic academic gains for children of suburban-moving families, the main benefit will likely only be to highlight the importance of family and social capital for student achievement. It is politically difficult to imagine that successful MTO experimentation would lead to a national commitment to relocate large numbers of inner-city families to suburban communities, though scholars may conclude that such relocation would be beneficial. After all, MTO experimentation can prove no more about the benefits of integrating low-income children with those more privileged than has been widely believed since *Brown v. Board of Education* asserted it in 1954.

Of as great importance, therefore, as MTO, are studies of the regular Section 8 program, or of low-income housing programs sponsored by Community Development Corporations

(CDCs) nationwide. Here, too, experimentation is possible because of extensive waiting lists for regular Section 8 vouchers or for places in low-income subsidized private projects. Section 8 is often considered a failure because many families utilizing vouchers may find housing which, while more adequate, is near the same "underclass" community where they lived before. In explaining the MTO experiment, the Department of Housing and Urban Development stated that "many Section 8 families continue to live in relatively segregated and economically distressed neighborhoods" (Goering et al. 1995). But this leaves an exaggerated impression. Many families benefit from Section 8 projects that, while requiring less radical change than suburban moves, substantially improve housing and community conditions. In 1989, 68 percent of families with Section 8 certificates or vouchers lived in census tracts where median family income was more than $20,000; 26 percent lived where median income was more than $30,000. In contrast, only 31 percent of public housing residents were in tracts where median income was more than $20,000, and only 9 percent were in tracts where it was more than $30,000 (Newman and Schnare 1997, Table 2). Sixty-seven percent of families utilizing Section 8 certificates and vouchers lived in tracts less than 30 percent minority, compared to only 38 percent of public housing residents (Newman and Schnare 1997, Table 4). Thus, much can be learned about impacts on student achievement from improvements in family capital (housing conditions) and social capital (neighborhood surroundings) of these regular Section 8 programs.

Experiments utilizing treatment groups that receive vouchers or project units in these cases, and control groups that do not, can shed light on the impact of stable housing itself on academic achievement, either because mobility is reduced, because space for study is more adequate, because family life is less stressed, or because those living in stable adequate housing have the opportunity, previously unavailable, to develop social capital in their neighborhoods.

There are three distinct issues for research to address. One is the disruption to education caused by moving from one neighborhood (or school) to another, especially when it happens frequently, and whether housing programs reduce this mobility and generate improved outcomes. A separate issue is identification of the negative influences, if any, on academic outcomes, of neighborhoods without great social capital, no matter how long a family remains. A third issue is housing quality, and the effect that poor resources (e.g., lack of study space, or greater stress from overcrowding) have on learning, regardless of the neighborhood in which a residence is located.

Briggs (1998) studied families who moved to scatter-site housing in Yonkers, as part of a court-ordered desegregated housing program similar to Gautreaux. He found that youth from segregated public projects whose families moved to more stable housing in a more middle-income neighborhood did not gain appreciably (in, for example, contacts that lead to jobs) from exposure to networks of employed adults in their new surroundings. Because selection of movers was not random, Briggs constructed a control group of "stayers" using chain-sampling or snowball techniques (interviews with movers leading to identification of controls). The

Briggs study differs from studies of both Gautreaux and MTO because Yonkers is a relatively small city without neighborhood schools, and both movers and public housing stayers continued to attend the same schools. Briggs, however, stimulated by Wilson's "truly disadvantaged" thesis, was interested primarily in exploring adolescents' job networks, and so did not investigate whether academic outcomes of these adolescents or of their younger siblings were affected by the move. His analysis suggests how studies of academic outcomes might be constructed.

In Albuquerque, scatter-site public housing is dispersed throughout a large metropolitan area, avoiding concentration of the poor in isolated ghettos that led to court-ordered Gautreaux and Yonkers voucher programs. Rusk and Mosely (1994) compared academic outcomes over a 10-year period (1983-84 to 1992-93) of public-housing children whose neighborhood schools were predominantly middle class to outcomes of similar children whose neighborhood schools were predominantly poor. They found that public-housing children attending schools with fewer than 20 percent of their classmates poor or near-poor (i.e., eligible for free lunch) had standardized test scores 13 percentile points higher than similar public-housing children attending schools with more than 80 percent of their classmates poor or near poor.[12]

Rosenbaum's analysis (1995) of Gautreaux suggests that children whose families use Section 8 to move to stable within-city housing do not gain educational benefits comparable to those in suburban-moving families. Early analysis of MTO's Baltimore experiment concludes that Section 8 vouchers, when used by families to remain within the city, do not have beneficial effects on educational outcomes like those of vouchers used in suburbs: "The main reason is that ... the schools are not much better" than those from which they came (Ladd and Ludwig 1997).

But Rosenbaum's measure of educational outcomes is limited to grade-point averages, college-prep course taking patterns, and college enrollment. Ladd and Ludwig rely on estimates of the quality of schools attended by experimental and comparison families, not on children's educational outcomes directly. More research utilizing better data on academic achievement (including test scores) is needed before firm conclusions can be reached about whether investment in housing would be an efficacious use of dollars intended to generate improved student achievement.

Further opportunities for research are presented by the nonprofit housing projects constructed in many urban areas by CDCs, often by combining Community Development Block Grants (federal funds distributed by local governments), local redevelopment set-asides, state bond funds, and federal low-income housing tax credits. Many of these projects are small, often located in the same communities where tenants previously lived in substandard dwellings. In most cases, there are extensive waiting lists for units, permitting comparisons

[12] This is the largest academic achievement effect of housing programs I have anywhere found described; to the best of my knowledge, it has not been published or peer-reviewed.

between outcomes of children from families who obtain units with outcomes of those whose families do not. Such comparisons potentially isolate the effects of stable housing itself, without confounding these effects with those of community changes, such as those experienced by MTO participants.

Special Education

Expenditures to improve family capital may also have the effect of *reducing* current school spending. In particular, an object of great school expenditure increase has been special education. Of new funds given to elementary and secondary schools from 1967 to 1996, 40 percent went to special education; only 23 percent went to regular academic programs. Special education now consumes 19 percent of all school spending (Rothstein 1995).

Only a small part of special education spending can be attributed to deficient family capital easily remediable by policy intervention, but this part still represents considerable spending; reducing it has the potential to offset the cost of environmental interventions. Special education categories most preventable by relatively low-cost environmental intervention are "learning disability" and mild (i.e., IQ from 70 to 95) mental retardation.

Noted above was the finding that smoking by pregnant women is associated with a 4 point decrease in their children's IQ scores. But the impact of smoking by pregnant women may have its greatest consequences for education finance in its impact on extreme cases, i.e., children categorized as "mentally retarded". An astonishing 35 percent of mental retardation cases in children are apparently caused by smoking in pregnancy (Drews et al. 1996). In 1995-96, 1.3 percent of all elementary and secondary students were classified as mentally retarded, 11.3 percent of all special education students (USDOE 1997); thus, approximately half of one percent of all U.S. schoolchildren suffer mental retardation from their mothers' smoking during pregnancy.

"Although the existence of learning disabilities is beyond dispute, the process of identifying students with these disabilities is fraught with complications. Definitions of the disorder are vague and broad..." (Terman et al. 1996). Learning disabilities are often difficult to distinguish from non-disabled slow learning, partly explaining the wide range in identifications nationwide. In 1995-96, 5.8 percent of all elementary and secondary students were classified as learning disabled, 51.3 percent of all special education students (USDOE 1997).

Learning disability, a category that also blends seamlessly into the category of non-disabled slow learners, "is one of the most sensitive barometers of the biomedical status of children and the psychosocial climate in which they live" (Hallahan 1992, 524). Poverty, substance abuse by pregnant women, nutritional deficiencies, toxicity (e.g., lead poisoning), and social instability may all contribute to learning disability. "Based on physiological factors, a certain percentage of the population is at risk for developing a learning disability. Given social support and/or a less stressful life-style, however, some of these individuals may function well

and never be identified as having a learning disability" (Hallahan 1992, 527).

We do not well understand how much the burgeoning cost of special education is attributable to accommodation of learning disabilities preventable with better nutrition, social support, and/or less stressful life-styles that might be enhanced by more stable housing. But assume that one-third of children presently identified as learning disabled could function normally if we made greater family and social capital investments, and assume that each such child now requires 1 to 2 hours a day of resource specialist time, perhaps in groups of six. Mildly mentally retarded children, one-third of whose cases, noted above, could be prevented by an effective campaign to eliminate maternal smoking during pregnancy, require similar resources. Assume further that resource specialists working with learning-disabled and mildly mentally retarded children, have typical caseloads of 24 (for example, in four groups of six that meet daily). If salary and benefit costs of typical resource teachers, along with associated support, diagnostic, and administrative services, are about $60,000 per year, then effective family and social capital investments might be offset by $2¼ billion annually in special education savings.

Conclusion

Levers to improve academic achievement may be found in family, income, and social policy as well as in schools. Educators have generally ignored these alternative levers, assuming that schools are more amenable to policy than families or communities. This belief may not be warranted. Not only have school improvements that generate large achievement gains, especially for disadvantaged students, been difficult to identify and implement, they are very expensive. Some relatively inexpensive interventions to improve family and social capital may have potential to generate larger academic gains. Policies to improve child and maternal prenatal nutrition, environmental conditions (lead contamination), or the age of mothers at first-time birth come immediately to mind. Reduction of smoking by pregnant women could have dramatic academic payoffs for their children. Raising the income floor of the working poor, either through tax or minimum wage policy, may also have measurable consequences in improved student achievement. Policies to improve the access of low-income families to adequate housing may be the most productive intervention.

Further study, however, is necessary before we can know with any degree of certainty whether these non-school interventions can, in fact, raise academic performance, or whether they can raise it sufficiently to be more cost-effective than school interventions on the horizon, like smaller classes and higher teacher salaries. This is not an argument not to invest in schools in ways that have demonstrated effectiveness. It is simply a recommendation that, when we consider cost-benefit implications of public expenditures to improve academic outcomes, a wider range of expenditure alternatives should be contemplated than is usually the case.

It would be wrong to interpret this report as endorsing a fatalist conclusion that schools cannot do a better job of educating disadvantaged children, and that policymakers should give

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up attempts to further improve the education of disadvantaged children until the social, cultural, and economic causes of low-achievement have been remedied. The academic outcomes of low-income and minority children have improved dramatically in recent decades, and the test score gap between advantaged and disadvantaged children has been narrowed, cut in half by some accounts. But, as with any major social change, the first half was easier to accomplish than the second half will be. The arsenal of tools to achieve this must be expanded considerably.

There is also growing evidence of particular schools that, because of excellent leadership, focus, and instruction, significantly surpass the academic accomplishments of other schools in similarly disadvantaged communities. It is commonplace to hear claims that such schools are distinguished by making "no excuses" for low achievement (Carter 2000; Barth, et al. 1999), that they have "broken the link" between poverty and achievement, or that they have "closed the gap" between the achievement of advantaged and disadvantaged children. However, evidence does not support these claims.[13] Some of the cited schools are not comparable to low achieving neighborhood schools in poor communities. As for others, while there have been some remarkable short-term test score gains in these schools, there is not yet evidence that they can be sustained. Disadvantaged children in such schools, while performing better than children in demographically similar schools, still do not achieve long term outcomes typically achieved by children from schools in more advantaged communities. In addition, claims that the gap can be closed simply by better educational practices utilize an inappropriate comparison. They compare exceptional schools for poor children with typical schools for middle-class children. But if exceptional school practices can improve academic outcomes for disadvantaged children, they can doubtlessly also improve outcomes for advantaged children as well. If all schools improve, it is likely that academic achievement of all children could be higher, maintaining existing demographic gaps but at a higher level.

It is entirely unproductive for advocates of community investments to blame schools for the persistence of achievement gaps, while advocates of school investments blame community deficits for these gaps. Clearly, if the achievement gap is to be narrowed further, and perhaps be closed, investments in both schools and social institutions are needed. School improvement, and investments in schools to generate that improvement, should not be removed from the agenda. But policy is crippled if it proceeds on the basis that only school improvement is needed to narrow the achievement gap. School improvement is unlikely to succeed if not

[13] As this report went to press, two instances of such claims were noted. In one, Kati Haycock, director of The Education Trust, commented on a just-released RAND report. "With targeted [school] investments and high expectations, we can close the achievement gap once and for all" (Wilgoren 2000). But an examination of the RAND report to which she referred (Grissmer et al. 2000) shows that while some states have narrowed the test score gap more than others, in even the most successful states, a substantial gap remains. A second instance was a commentary in the journal *Education Week* (Schorr). Here, Lisbeth Schorr claims that some "schools and districts did succeed in breaking the link between inner-city poverty and school achievement—and on a scale large enough to matter," but the author provides no evidence to support this claim.

complemented by improvements in other, equally important educational institutions: families, communities, and the social and economic environment.

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SCHOOL VS. NON-SCHOOL INVESTMENTS FOR SCHOOL ACHIEVEMENT: IS A CHOICE NECESSARY?[1]

Robert E. Slavin

In "Finance Fungibility: Investigating Relative Impacts of Investments in School and Non-School Educational Institutions to Improve Student Achievement," Richard Rothstein poses a provocative hypothesis, namely, that investments in a variety of non-school interventions may have greater impacts on student achievement than widely recommended school investments, such as reducing class size and increasing teacher quality. In his view, investing in low-income housing, prenatal and child health programs, income maintenance, and other means of increasing "family capital" and "social capital" could be the most cost-effective forms of school reform. Rothstein starts with a figure of $24 billion per year as a cost of increasing per pupil expenditures by $500-per-child to reduce class sizes, increase teacher salaries, or improve teacher education.

The potential impact of such interventions as reducing lead poisoning, reducing family mobility, or improving early health care on school performance, or at least IQ (not at all the same thing), are well known, if not always well documented. Few educators would deny their importance or oppose public expenditures to combat these scourges associated with poverty. However, where Rothstein breaks new ground is in suggesting that these interventions are *more* effective for school achievement than school reform itself.

Rothstein's argument depends on a number of wild statistical assumptions. A key example is the use of the $24 billion figure for school reforms applying to every child in the United States, while his estimates relating to improving the social and family capital of poor families affect only these families. To compare apples and apples, it would be necessary to assume not a $500-per-child increase in expenditures, but an increase of $2,632 for the 19% of students living in poverty, or reducing class size only for these children at a total cost of only $4.56 billion.

The relationship between potential non-school interventions and school achievement is rarely demonstrated directly, and even more rarely demonstrated in longitudinal experiments, as Rothstein notes. Still, some of his proposed investments, such as nutritional supplements and smoking reduction interventions for poor mothers, seem so sensible and cost-effective that they would make sense even if they had no lasting effect on school achievement.

Another problem with the basic assumptions underlying the article is that while

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Rothstein is willing to posit all kinds of complex and unproven non-school interventions (e.g., increased access to Section 8 housing reduces mobility, which increases achievement), he is unwilling to compare alternative school-based interventions. At $500 per pupil, an elementary school of 500 students has $250,000 per year to spend, and a secondary school of 1,000 students has $500,000. With this much money, schools could adopt any number of effective programs, from tutors to comprehensive whole-school reform models to extensive professional development in proven curricular models (see Slavin and Fashola 1998). Class size reduction is preferred by politicians only because it is easier to understand and more popular than these far more cost-effective interventions, but that is no reason to accept it as the standard against which other investments should be compared. Just to play out one of these possibilities, imagine that $24 billion were used to hire 480,000 certified teachers, who could provide 30-minute one-to-one tutoring sessions to about 4.8 million children every day, about 20% of all elementary children. In other words, every poor child in every school in America could have a tutor every day for their entire 6 to 7 years in elementary school. Research on tutoring by certified teachers (e.g., Wasik and Slavin 1993) would anticipate an achievement effect size[2] of +0.75 or more for the children served, much more than the estimates Rothstein gives for reductions in class size or increases in teacher quality.

Rothstein provides a real service in reminding us that we should include all possible interventions, not just school interventions, in considerations of the cost-effectiveness of alternative public policies. Educators are certain to emphasize educational solutions, just as health professionals would emphasize health solutions and housing professionals would emphasize housing solutions. However, posing interventions in each of these areas as being in competition with each other forces a choice that is unnecessary. If we are serious about the educational problems of disadvantaged children, then we will intervene on all of these fronts with the most effective and cost-effective tools we have at our disposal.

[2] Proportion of a standard deviation by which a treatment group exceeds a control group. Rothstein cites estimates of achievement effect sizes due to increasing per-pupil expenditures by $500 ranging from +0.04 for reducing class size to +0.22 for increasing teachers' education levels.

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IMPROVING THE EFFICIENCY OF PUBLIC EXPENDITURES DIRECTED TOWARD CHILDREN

David W. Grissmer

The thesis of this paper suggests that (1) improving the achievement and educational outcomes of at-risk students will require significant investments outside schools, and (2) that these investments outside of schools may be more efficient than investments in schools. These are very important issues to address, and the paper does an excellent job of framing the issues more clearly and succinctly than previous efforts. The author's excellent and wide-ranging literature review citing measurements of educational effects outside the common schooling variables is bound to widen the scope of discussion, research, and policy focus.

The issues are important partly because of the substantial and growing societal investment in children. Governments at all levels spend over $500 billion annually directed toward children of ages 0 to 18 (Office of Science and Technology, 1997).[1] Approximately two-thirds of this is for direct K-12 educational expenditures, while the remaining amount is directed primarily toward family assistance of various types and criminal justice system expenditures for juveniles. Besides government expenditures, families spend approximately $500 to $600 billion annually on children between ages 0 to 18.[2] Even modest increases in efficiency in this spending would have significant payoff.

This investment is disproportionately targeted toward higher SES children. Private spending per child depends on the level of family income with children in higher-income families receiving significantly more expenditures. Public expenditures per pupil on K-12 education also show higher amounts for children from higher SES families.[3] Only public spending on family assistance is primarily directed toward lower SES families. The net result of both private and public spending on children is still a significant gap in net spending on lower SES children. Significant gaps also exist in achievement between minority and non-minority children and higher and lower SES children (Jencks and Phillips 1998). The questions posed by the author about these inequalities are:

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[1] This estimate does not include the forgone taxes from child deductions, day care, and other deductible items related to children, nor does it include housing subsidies.

[2] This amount is estimated assuming the cost of raising a child to age 18 to be approximately $150,000 and assuming approximately 70 million children between 0 to 18. (United States Department of Agriculture, 1997).

[3] Per-pupil expenditures (cost of living adjusted) across states varied from approximately $4,000 to $9,000 per-pupil in 1993-1994, with poorer states spending less. In addition to between state disparity, there is also disparity in within state school district expenditures between wealthy and poorer districts across most states. A few states have nearly equalized expenditures across districts after successful litigation (Carr and Fuhrman 1999). However, most states still have significant variation in per-pupil expenditures across districts (Parrish and Hikido 1998). Variation across states accounts for about two-thirds of the total variation, while within state variation accounts for only two-thirds of the variation (Evans et al. 1999).

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- Whether these differences in expenditures are related to different educational outcomes;
- Which specific types of expenditures (school vs family) are more strongly related to educational outcomes;
- Whether some expenditures benefit achievement of lower SES children more than higher SES children;
- Whether there are substitution possibilities in different types of expenditures in achieving better educational outcomes (i.e., can school and other public expenditures overcome the advantage of higher private family investments in higher SES children); and
- What the "optimal" mix of expenditures might be.

While these questions address some of the underlying economic issues, perhaps as important are the political and legal issues inherent in these questions. For instance, it may be the case that a significant closure of the gap in achievement between low and high SES children is possible, but only by public spending that allocates much higher expenditures per pupil to lower SES children. Are there any political circumstances or a legal basis that would allow such expenditures to occur, or is it inherent in our legal and capitalistic system that, at best, equality of spending is possible. Certainly, some worry whether the political consensus for support of public schools is possible with much larger allocations to at-risk children.

I think that the author rightly concludes that the focus of *educational* research and policymaking has been too narrowly focused on school investments in the quest to improve achievement and educational outcomes. However, it is not the case that research and policymaking has lacked a wider family focus. As indicated above, the public sector probably spends as much as $200 billion a year in various forms of support for lower SES families. While these expenditures are often not thought of as improving educational outcomes, they actually improve educational outcomes. AFDC, food stamps, Medicaid, prenatal programs, child abuse prevention, adoption support, teen pregnancy—all of these programs are improving educational outcomes for children, although the programs often have broader goals as well. Research on these programs has tended to focus on their effects on different objectives, such as future welfare utilization and employment. I think the author is rightly arguing for a *conceptual* shift in both the research and policy world to see these types of expenditures, along with the $350 billion spent on K-12 education, in a more unified way.

However, there are two ways to achieve more efficient allocation of funding to improve educational outcomes of at-risk children. The author suggests that there may be a better mix of program expenditures—especially when we consider the broader mix of programs suggested—that could achieve better results with current total budgets. There is also an allied way of achieving more efficient allocations, namely, by better identification of which children

are truly at-risk, and better targeting of funds to these children. This question is intimately connected to *who controls and allocates* funds and whether funds are fungible across programs, rather than the "optimal" mix of funding.

Until recently, the federal government maintained the primary policymaking and funding role with respect to family programs, while the state and local government maintained a similar role for educational expenditures. Now, for the first time, all of these programs are primarily the responsibility of the states. The states now have the pivotal role in policymaking with respect to both family and school expenditures for at-risk children. States would be expected to take different approaches to beginning to integrate the policymaking across these areas. Having 50 states take different approaches can have a significant advantage if good research can evaluate what approaches are working best.

For instance, some states are moving responsibility and providing more fungible funding across programs to lower levels. In at least one case (North Carolina) some of these expenditures are being combined and provided, not to direct governmental agencies, but to local county-level quasi-nonprofit/governmental entities. These entities have broad authority with fungible funds to allocate across a broad range of services that reflect the needs of the specific locale. Local entities may have significant competitive advantages in efficiently allocating funds to help at-risk children. The children/families and schools in different locales may have distinctly different needs, and local entities can probably better identify these needs, and if funds are somewhat fungible, a more efficient mix of programs may be funded. For instance, health care or after-school care or teen pregnancy prevention may each be severe problems in some communities, but not others. It is probably at the local level with fungible programs that the type of broader integration and more efficient mix of programs envisioned by the author might be achieved.

Besides better identification of needs, local authority has, I think, an even more important advantage. It is still hard to identify at-risk children based solely on family characteristics. For instance, most children in low-income families do not score in the lowest quartiles of achievement. Moreover, the literature on risk and resiliency shows that about one-half of children from the poorest family environments will become adults with jobs and families, and without severe dysfunction.[4] This literature tells us that determining which children are at-risk is a difficult undertaking. Often, children in the same family environments will have quite different outcomes, so some children in a family will be at-risk, while others are not.

Children near the bottom of the achievement score distribution are best identified by multiple risk indicators. It is the combination of low parental education, low income, large family size, and living with a single parent that are much better predictors of risk. An

[4] See, for instance, Rutter 1988 and Masten 1994 for introductions to this rich literature on risk and resiliency in children.

indication of this is that most—but not all—children of unmarried teen parents have among the poorest educational outcomes. These children are most likely to be at multiple risk, that is, to live with one parent, have low income and low parental education, and are often from larger families. Funding of national and state programs such as AFDC, food stamps, Medicaid, Title I, and housing programs are usually allocated based on a simple income measure. Since income alone is a poor predictor of at-risk children, these programs are likely to be less efficiently targeted than programs that can take into account a more comprehensive set of measures available at local levels.[5] In addition, it is often indicators not available to any government—such as mental health and addiction conditions in parents—that better predict at-risk children and can only be known in the local environment.

The consequence of the difficulty of identifying at-risk children with simple indicators like income level is that state and national programs directed toward at-risk children could be inefficient compared to the programs that could be better targeted at local levels. Recent research has suggested that the cost-effectiveness of programs is very sensitive to which groups are targeted (Karoly et al. 1998). A visiting nurse program was very cost-effective for a group of teen mothers with the lowest level of resources, but not cost-effective for a group of mothers with higher levels of resources. Certainly, the best judgment of being at-risk can be made by professionals who know individual children and families and can take account of multiple indicators. Such individuals can also better assess the needs of such children and families.

While local entities comprised of professionals may have the most potential for the efficient allocation of funds, there can be inefficiency at this level also. Local politics can certainly introduce inefficiency into the process. The reason for the North Carolina Smart Start program being governed by a quasi-public/private entity may be to avoid this kind of inefficiency. Some locales may not have the kind of multi-faceted expertise to make efficient judgments. State politics can certainly still make inefficient allocations across locales. However, I believe that the difficulty of efficiently identifying which children are at-risk makes local control potentially the most efficient allocator. Certainly, much research and experimentation is needed both in how to efficiently identify at-risk children, and what institutional arrangements seem to produce the most efficient allocations.

Another comment concerns the methodology necessary to make the kinds of policy decisions among a broader mix of school, family, and community programs to improve educational outcomes. One reason for the narrow focus of educational research is that the objectives of the programs are more easily measurable and available. Either achievement or high school completion or total years of education are the common measures used to evaluate the effectiveness of schooling expenditures. While these narrower objectives makes

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[5] A second reason why these programs may be inefficient is that the political process inevitably widens the target group to include higher income levels.

for easier and tidy research, these are not the ultimate objectives we are interested in achieving. Ultimately, we must connect both family and school expenditures to the effect on longer-term contributions (taxes raised) and disbursements (welfare, criminal justice, Medicaid, etc) of public expenditures. Better educational outcomes presumably contribute to higher wages and more taxes, and to reduced social welfare, health, and criminal justice expenditures.

Using these objectives is necessary for at least two reasons. Most family programs have and accomplish a wider set of objectives than improved educational outcomes for children. They are directed at reducing the chances of delinquency, reducing welfare utilization, and boosting wages for parents. Thus, comparisons of schooling and family expenditures to be comparable must use common measures of objectives. Both types of expenditures eventually hope to produce adults (either current parents or current children as adults) with stable employment and reduced utilization of social service and criminal justice programs. Research is moving in this direction of comparing the long-term savings from investments in family or school programs.[6]

A second reason for moving in this direction is that the "optimal" level of investments in all children's programs can never be determined without this methodology. An intermediate measure like achievement can be used to compare the relative cost-effectiveness of programs, but can never determine how much should be invested across all programs.[7] The total investment level can only be determined by comparing the discounted costs of the programs to the discounted net savings in future public expenditures and revenues, and estimating a rate of return. A rate of return that is higher than achieved in private-sector investment would argue for increased expenditures.

This rate of return is likely to differ significantly across family and student SES levels. Recent research suggests that inefficiency in previous K-12 spending may be primarily for funds directed toward higher SES students. Funds directed toward minority and lower SES students have generated significant achievement effects and are the most cost-effective when using achievement as the objective (Grissmer et al. 1997; Grissmer et al. 1998; Grissmer et al. 2000). An intriguing possibility is that using longer-term measures would show significant rates of returns for such investments directed toward lower SES children. In such a case, higher levels of funding for such programs would not depend on altruism, but would be mandated by societal self-interest.

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[6] See Karoly et al. 1998 for comparisons of the costs and long-term savings from one early intervention, and Krueger 1999 for estimates of the long-term impacts of class size reductions on wages compared to costs.

[7] See Grissmer et al. 2000.

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THE IMPORTANCE OF NON-SCHOOL INVESTMENTS FOR IMPROVING SCHOOL READINESS AND EDUCATIONAL OUTCOMES

Ann Segal

This paper is both timely and presents an important set of issues as policymakers and the American public focus attention on improving school outcomes for all of our country's children. School outcomes are crucial for the life success of this generation's children, for the life success of the children that the current generation of children raise, and for the strength of this nation. And, clearly, our children could be achieving more. The paper points to a number of steps that could be taken and to the fact that there are always trade-offs within a limited budget.

The First Education Goal, having all children arrive at school ready to learn, must be understood to mean that children should not only arrive their first day at elementary school ready to learn; they must also arrive every day throughout their school years ready to learn. This is not only the first, it is the most crucial of the education goals. It recognizes, as does this paper, that children who are hungry or sick or living in families in which parents cannot or will not function as strong, supportive, and loving parents cannot be expected to score well on standardized tests or complete their homework assignments. Children's learning cannot be separated from their overall well-being. While it is certainly important to ensure the best teaching and learning environments within schools, it is vital to ensure that the children who enter schools are able to engage education fully.

The First Education Goal and this paper therefore propose correctly that steps that must be taken to improve school outcomes may be outside the school walls and outside the school budget. Since good parenting is the most important ingredient of any child's well-being, a fact rooted both in common sense and supported by research, the best answers for improving school achievement may lie in strengthening families. Children's outcomes would certainly benefit if their families were not living in poverty, had stable and safe housing, and had access to health care and food for all family members. It is also well known that preventing teen pregnancy, children's exposure to lead, asthma crises, and the effects of prenatal exposure to alcohol, drugs, and tobacco would do much to help children succeed. Schools can be partners with numerous organizations and individuals at the federal, state, and local level in addressing many of these issues, but schools cannot overcome their impact for many children.

There are a number of issues that the paper might have addressed more fully. I will briefly identify three. The first is the differential impact of many influences based on the age of the child. It is important to recognize that it is crucial to look squarely at our youngest children. Poverty experienced very young is more damaging than poverty experienced later in life. The effects of a lack of health care or poor nutrition can be more devastating during

the youngest years, when children's bodies and minds are developing more quickly. Some of the effects of prenatal exposure to drugs can be overcome with appropriate, early interventions. While older children continue to need the same supports for optimal development, great opportunities are lost if significant investments are not made for prenatal care and for infants, toddlers, and preschoolers.

Secondly, the paper does not stress the reality of young children's lives today. Many spend numerous hours in child care settings beginning in infancy. Research is consistent in findings of the importance of ensuring that substitute care, like parental care, must be of good quality. And it is obvious that poor-quality care, especially unsafe and unloving care, will lead to poor outcomes, including a lack of school readiness. Ensuring quality care for all children from infancy is another major opportunity for investment for school achievement.

Third, the paper attempts difficult, perhaps impossible, analyses around budget trade-offs. Not all problems or solutions, outside or inside schools, are universal. If analyses tried to present dollars targeted to known, effective interventions for certain problems experienced by certain children, they would be incomplete due to a lack of good data. Obviously, too, such analyses would suffer from political realities. Budget trade-offs are not made simply by lining up budgets to see what is most cost-effective. Values and lobbying influences are always present. Indeed, targeting efforts on identified groups of children may even be politically impossible if there are pressures to have universal responses, even though this may limit the intensity of individual interventions to the point of ineffectiveness. One way to balance concerns may be to make investments for all while varying the intensity of interventions according to need.

Despite the limitations of the analyses presented in this paper, its message is rightfully provocative. Those who want to increase school success for all children must consider the whole child or they will fail. Educators should be among the strongest voices for investments outside the school walls, since they will benefit greatly from classrooms filled with children truly ready to learn. With help, all children can be successful in school and become good, productive citizens. Wise investments support these outcomes.

NO SILVER BULLET: QUESTIONS AND DATA ON FACTORS AFFECTING EDUCATIONAL ACHIEVEMENT

Thomas F. Pogue

In his paper, Professor Rothstein makes two important contributions. He explains that dollars spent on income maintenance, nutrition, health, housing, and community infrastructure may be as effective in improving student achievement as dollars spent on schools. And he urges those engaged in educational research and policy to investigate the trade-off between school and non-school spending as means of improving achievement.

My comments build on this base by providing evidence on how various school and non-school factors appear to affect student achievement, as measured by scores on the ACT (American College Testing) Assessment. ACT scores are relevant measures of student achievement because they reflect the cumulative effects of elementary and secondary education; they are a "capstone" measure of academic achievement.

The regressions in Table 1 clearly show that non-school interventions are, as Rothestein suggests, potentially important means of increasing achievement (scores). For example, factors associated with low family income reduce the scores of African-American students on average by 0.87 points, holding constant the influences represented by school-district income, school spending, courses taken, and other variables in the regression. Eliminating the adverse effects of low family income would therefore increase the average score of African-American students by 0.87 points, which is 0.26 standard-deviation units (SDU). The increase for white students would be less, 0.41 points, so eliminating the effects of low family income would reduce the "black-white" test score gap. Eliminating the effects of high school-district poverty (poverty rate of 30 percent or more) would also reduce this gap, since it would increase scores of African-American students by 0.80 points and white students by 0.15 points.[1] Reducing family and school-district poverty would also improve achievement by reducing the percentage of students qualifying for subsidized lunches. A 10 percentage-point reduction in students qualifying for subsidized lunches would increase scores by 0.13 and 0.16 points for African-American and white students, respectively.

Unfortunately, it is unclear what actions should be taken to eliminate the effects of low family income. As Rothstein explains, we do not know whether the cause of low scores is low income *per se* or other influences correlated with low income, such as poor nutrition, poor health, and poor parenting. Likewise, we do not know the mechanisms by which high

[1] The sample mean scores are 21.9 and 16.7 points for white and African-American students, respectively. The corresponding standard deviations are 3.4 and 4.3 points. The black-white test-score gap based on average scores is 5.2 points. The gap will differ from the sample average depending on students' characteristics and environment. For example, the gap will be larger than average for poor students attending schools with high minority enrollments in school districts with high poverty rates. It will be smaller than average for high-income students attending schools

school-district poverty adversely affects achievement. Furthermore, we do not know how much it would cost to eliminate the adverse effects of either low family income or high school-district poverty; we do not have the information needed to assess the cost-effectiveness of these interventions.

The regressions also point to several potential school-based interventions to increase achievement. A $1,000 increase in per-pupil spending is associated with an average score increase of 0.17 points for African-American students and 0.14 points for white students. The effect of spending is small quantitatively, although statistically significant, in part because other variables that reflect spending—pupil-teacher ratio, number of students taking advanced courses, magnet schools—are included in the regression. A decrease in the pupil-teacher ratio of 10, say from 25 to 15, is associated with an average score increase of 0.61 and 0.14 points for African-American and white students, respectively. The dollar cost of increasing spending and decreasing the pupil-teacher ratio for a given set of students can be estimated fairly easily. But we cannot determine whether these interventions would be more cost-effective than the non-school interventions discussed above because we do not know the cost of the latter.

Providing students with the background needed to take high-level courses and encouraging them to do so is another potentially important school-based intervention. Holding constant the effects of family and school-district income and other variables in the regression, scores are on average 0.87 + 0.96 + 0.15 = 1.98 points higher for African-American students who take four or more years of math and science than for students who do not. The corresponding difference for white students is 2.25 points. Similarly, students who take advanced placement and honors courses score higher than those who do not. These regression coefficients may, however, overstate the positive effects of taking advanced courses, because students with relatively high (low) ability/motivation are more (less) likely to take advanced courses. This selection bias may also result in an overestimate of the effect of magnet schools; the more able and ambitious students may seek out magnet schools.

Scores are significantly lower for students attending schools in which their classmates are predominately minority students. The effect is greater as the fraction of minority students increases, and it is greater for African-American than for white students. Why this is so is unclear. One possible explanation is that schools with high minority populations have fewer and/or poorer quality resources than other schools, even when per-pupil spending is held constant. Another possibility is that peer effects are unfavorable; peers may not place high value on academic achievement and may not regard college as a realistic goal. Whatever their causes, offsetting the adverse effects of segregation could increase scores — by an average of 0.78 points in the case of African-American students attending schools with 75 percent or more minority enrollment. But we do not know what school and non-school interventions

with low minority enrollments in low-poverty districts.

would achieve this end.

The regressions reported in Table 1 do not, of course, include all of the factors that might influence achievement. But they do include many of the commonly discussed factors. And some potentially important but omitted factors may be correlated with included variables. For example, family income is correlated with and may proxy for parents' education, which is not included but generally thought to affect student achievement.

Only 30 to 35 percent of the variation in test scores is associated with variables included in the regressions of Table 1; much of the variation in achievement remains "unexplained." Further, no variable can be singled out as a major determinant of test scores. Instead, observed variation in scores, to the extent that we have succeeded in explaining it, can only be attributed to many small influences.[2] The policy implication is that there is no "silver bullet" for improving educational outcomes.

I will conclude by noting that perhaps the most important contribution of the Rothstein paper is the questions that it defines for future research in school finance: 1) Which areas of non-school spending have potentially important effects on academic achievement? The results reported in Table 1 are evidence on this point. 2) Is there evidence that *some* non-school programs are more cost-effective than *some* school programs as means of improving academic achievement? That is, is there evidence that academic achievement can be improved by reallocating resources (dollars) from a school program to a non-school program? This is the policy issue posed by Rothstein's paper. 3) How can researchers obtain evidence of the sort referred to in the preceding question? How can they determine whether specific non-school programs are in fact more cost-effective than specific school programs or activities?

[2] The same is true for related studies: Card and Krueger (1996), Ferguson and Ladd (1986), Downes *et al.* (1998), Card and Payne (1998), Sander (1999), and the studies reviewed in Hanushek (1986 and 1996).

Table 1. Estimated Effects of School, Community, Family, and Student Characteristics on ACT Composite Test Scores

Table 1 reports OLS (ordinary-least-squares) estimates of the following equation:

$$(1) \qquad S_i = a_0 + b_1X_{1i} + b_2X_{2i} + \ldots + b_nX_{ni} + e_i$$

where S_i is the ACT composite test score of student i, and $X_1 \ldots X_n$ are the independent variables in Table 1. The regressions were estimated from data on 19,868 African-American and 124,860 white students who attended public schools in Louisiana, Minnesota, Mississippi, and Wisconsin and were scheduled to graduate in school years 1995-96 and 1996-97. This sample is 37 percent of the graduates in those states and years. The states were chosen for two reasons: 1) a high percentage (66 percent) of high school graduates in these states took the ACT Assessment, and 2) MN and WI have high average scores while LA and MS have low average scores.[3]

(Note: regression coefficients in **bold**; t-ratios below coefficients)

	Regression Coefficient	
Independent variables	African- American	White
Family and community		
Low family income, $36k or less[a]	**-0.869**	**-0.413**
	-16.2	-18.5
High family income, $80k or more[a]	**0.444**	**0.275**
	2.9	7.9
English not primary language in home[a]	**-0.921**	**-0.582**
	-3.9	-3.8
Female[a]	**0.068**	**-0.387**
	1.7	-19.2
School district poverty rate: 16 - 29.9 %[a]	**-0.589**	**-0.029**
	-4.7	-0.8

[3] More detail on data and estimation procedures can be found in Thomas F. Pogue, James Maxey, and Chia-Hsing Liu, "Outcomes of Public Education: Weighing the Effects of Dollars, Family, Peers, and Community," in Proceedings of the 92nd Annual Conference on Taxation, National Tax Association: Washington, DC, 2000 (forthcoming).

School district poverty rate: 30 % or more[a]	**-0.798**	**-0.150**
	-5.7	-2.6
Percentage of population without high school diploma	**0.004**	**-0.025**
	1.1	-13.6
Rural; non-metro location[a]	**0.333**	**0.294**
	4.4	10.1
Urban location[a]	**-0.235**	**0.052**
	-3.3	1.7
School characteristics		
Per-pupil expenditure ($1000)	**0.170**	**0.138**
	3.0	6.6
Pupil-teacher ratio	**-0.061**	**-0.014**
	-3.1	-3.4
Magnet school[a]	**1.379**	**2.020**
	17.6	19.1
School offers one or more AP courses[a]	**-0.034**	**0.024**
	-0.6	1.0
School with 50% to 74.9% minority students[a]	**-0.339**	**-0.294**
	-5.6	-5.1
School with 75% or more minority students[a]	**-0.775**	**-0.317**
	-10.3	-2.2
Percentage of students qualifying for subsidized school lunch	**-0.013**	**-0.016**
	-6.9	-11.5
High school class < 100 [a]	**-0.502**	**0.110**
	-8.9	3.6
High school class > 400 [a]	**-0.366**	**-0.179**
	-6.2	-6.7

Table 1. (continued) Estimated Effects of School, Community, Family, and Student Characteristics on ACT Composite Test Scores

(Note: regression coefficients in **bold**; t-ratios below coefficients)

	Regression Coefficient	
Independent variables	African- American	White
Courses taken		
4 or more years math[a]	**0.872**	**0.978**
	17.0	34.0
4 or more years science[a]	**0.957**	**0.902**
	11.9	23.5
4 or more years of math and science[a]	**0.149**	**0.370**
	1.5	8.1
Advanced/honors work in English[a]	**1.270**	**1.932**
	24.1	76.8
Advanced/honors work in math[a]	**1.037**	**2.232**
	17.7	83.5
Advanced/honors work in science[a]	**0.380**	**0.771**
	5.6	25.3
Advanced work in Eng., math, & sci. [a]	**0.096**	**-0.107**
	1.0	-2.5
Intercept, state effects		
Intercept (Louisiana)	**17.698**	**18.474**
	43.7	168.5
Minnesota[a]	**0.689**	**1.515**
	4.1	30.4
Mississippi [a]	**0.329**	**0.808**
	4.5	14.5
Wisconsin [a]	**0.684**	**1.612**
	4.7	29.6

Number of observations	124831	19839
Multiple R	0.596	0.560
R Square	0.356	0.314
Adjusted R Square	0.355	0.313
Standard Error	3.484	2.796
F	2459.125	324.230

[a]Indicator variable that takes on a value of 1 if the student is in the indicated category; otherwise it is zero. For example, when the low-family-income and high-family-income indicators are both zero, the student is in the middle-income category ($36k to $80k). When all indicator variables are zero, the student is a male from a middle-income, English speaking, suburban family who is attending school in a Louisiana school district with a poverty rate below 16 percent. The school is not a magnet school, it does not offer AP courses, it has minority enrollment below 50 percent, and the high school class size is between 100 and 400. The student has not taken 4 or more years of math and science, nor has he taken advanced placement or honors courses.

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